DISCUSSION BEFORE THE WIRELESS SECTION, 2 JUNE, 1926.

Mr. P. K. Turner: There is in my opinion a fundamental objection to the bridge method described by the author. A few months ago, when the author described this bridge in connection with another paper, he claimed as one of its great advantages that the transformer was measured under normal working conditions. But I perceive that under the usual conditions of use of this bridge there is no alternating current in the anode circuit of the valve following the transformer being measured. Hence there is no throw-back across that valve. From this it follows that there is no possibility of negative resistance in the input circuit of that valve, and hence the transformer has quite definitely a positive load on its secondary winding. If there were an inductive load in the anode circuit of the valve following the transformer, it is practically certain that the effective load on the transformer would be considerably reduced, with perhaps quite different results on measurement. In everyday use, the valve following the transformer will have in its anode circuit either another transformer or a telephone (or loud-speaker); in fact, there will be in this circuit a complex impedance. Therefore at different frequencies there will be a varying load on the transformer, which will affect its anode impedance and almost certainly its performance. Thus it seems to me that the bridge does not, in fact, fulfil the stated conditions of measuring the transformer under conditions of use. Now that we have received so much information about what one might call the preliminary design of a transformer, I am anxious for even more information. Supposing that by the use of the author's principles of design one has arrived at the point that the primary of the transformer which one proposes to design must be of such and such an inductance, that the magnetic leakage must not be more than a certain small percentage, and that the stray capacities must be below a certain small amount, how does one proceed to design the transformer? What is the permeability of an iron core with laminations of normal thickness under conditions of use in a transformer? I can think of three possible permeabilities, owing to the fact that there is a steady polarizing current in the core. There is the ordinary permeability, B/H, measured at the value of H given by the pure alternating current; there is the same B/H measured at the H given by the polarizing current; and there is the differential permeability, dB/dH, measured about the H given by the polarizing current. With most types of iron these will be widely different from one another. Then, again, what is the effective cross-section of the iron with laminations of normal thickness, i.e. to what extent does the field penetrate into the iron itself at various frequencies? Again, in view of the fact that the transformer works with a high resistance in its primary circuit, what is the comparative importance of transformer losses? I am sure that information on some of these puzzling points would be much appreciated by all of us, and I should like to make it clear that any criticisms which I have made on details of the author's methods do not in any way indicate a lack of appreciation of the very valuable work that he has done.

(*Communicated*): Although it may seem only a minor detail, I should like to comment upon some of the symbols used in the advance copies of the paper, especially the use of ρ* for the internal resistance of the anode circuit. There is a general tendency in physical work nowadays to use small Greek letters to indicate entities which are essentially in the nature of absolute quantities, while using English letters for actual physical quantities: typical of this is the author's own choice of σ for the step-up ratio of the transformer, which is a pure number; and of L and R for inductance and resistance, which are physical quantities. Apart, however, from this general objection, I should like to call the author's attention to the fact that ρ has been officially standardized as the symbol for resistivity, and should therefore not be used for a resistance. The symbol R_a is almost universally used as the symbol for internal differential resistance between anode and filament of a valve. There are other points of termin-

* Corrected for the *Journal* and the *Proceedings*.

ology in the paper to which I should like to refer. For example, immediately under Fig. 2 the author indicates by Z what he calls the "anode impedance," but judging by the analysis which follows, what he really means by Z is the *external* impedance of the anode circuit. Again, in the same sentence he denotes by v_a the "anode voltage", and again the analysis shows that what he means is the voltage developed in the external anode circuit, which is a quite different thing, for "anode voltage" should surely mean the total E.M.F. induced in the anode circuit. Again, on page 167, and I believe elsewhere, he refers to the "coefficient of coupling," and gives it as 1 per cent.* It seems fairly obvious from the context that he really intends to say that the coefficient of coupling is 99 per cent and that the *leakage* coefficient is 1 per cent.

Mr. H. A. Thomas: I wish to confine my remarks entirely to the method of measurement. My objection to the method is that the output valve is not fulfilling its normal functions. It is not giving the load on the transformer secondary; and, as is well known, the effect of very small impedence shunts across the secondary is very serious. The backing-off valve which is used acts virtually as a shunt to the normal output valve, and the grid of this valve is connected by a large amount of apparatus forming a large input capacity to the other output grid. I have recently tried the effect of this shunting valve as shown in the author's diagram. A Marconi transformer was used with a D.E.R. valve and 100 volts high tension, and a negative grid bias of 4 volts, the frequency being 2 000. The stage amplification with a weak output under normal conditions was found to be 33. A similar valve was connected across the original output valve in the same way as shown in the diagram, and the amplification fell to 28. The connecting grid lead was lengthened to about 1 ft., which I should estimate would be about the length that would be used with the bridge. The amplification fell from 28 to 26·5, showing very clearly that even a small amount of wire will have a very great effect on the amplification. The effect of the capacities of the bridge taken as a whole, not to earth, would increase this fall. Therefore, it seems that the presence of the backing-off valve may produce an error of at least 25 per cent in the determination of the amplification. I should be interested to know what differences the author has obtained with his method when compared with other methods which give normal output conditions.

Mr. H. L. Kirke: The work I have done on transformers has been concerned with input and output transformers for amplifiers used in broadcasting transmission chains rather than intervalve transformers; but some points do actually come into the question in connection with intervalve transformers. The author discusses the series-condenser method of increasing the low-frequency component of amplification in any intervalve transformer system. I have used that method, and I believe that other wireless engineers have used it in various connections, particularly in the case of output transformers, where the output is connected to a line. I have found it very satisfactory indeed, and I have taken care to see, by the use of cathode-ray oscillographs and the like, that there is no distortion whatever. With regard to the question of iron distortion, the point did arise, as I was using transformers at very high flux densities, and the cathode-ray oscillograph showed that there was no appreciable distortion in the iron, provided that a value of B of 5000 per cm^2 was not exceeded. In this connection it is interesting to know that the value of inductance of a transformer, i.e. the effective value of permeability, can vary about 15 to 1, according to the value of flux density in the iron over the range; but, nevertheless, the minimum permeability appears never to decrease below the value of between 200 and 300. Those tests were made with the ordinary inductance bridge, amplifying the output of the bridge in a carefully screened amplifier, and putting various known values of the current into the bridge so that the actual value of H could be calculated. One point brought out in the paper, in connection with the series-condenser method of bringing up the low frequency, is the variation of output impedance due to the condenser and transformer primary forming a series resonant circuit, lowering its impedance about the resonant frequency. That question is of importance, particularly if the input to the valve is large; the dynamic characteristic will vary considerably and so cause distortion. Although it is not actually a point in connection with this paper, the distortion due to the use of reactive output impedance in a rectifier is very interesting. In some forms of rectifiers, particularly the crystal, the effect of input impedance on rectification is dependent very largely upon the low-frequency back E.M.F. If that back E.M.F. is out of phase with the low-frequency current, some curious effects will be noticed. I have noted them on the cathode-ray oscillograph: in one particular case I had a rectifier which should have been perfect, but the oscillograph showed that the results were very far from being perfect. That is due to having a reactive impedance. On making the reactance completely non-reactive, the distortion disappeared completely. The author has pointed out that, by placing a condenser across the primary of the intervalve transformer, he effectively turned it into a low-pass filter. He stated that the value of the condenser should be one-third of the effective capacity of the secondary winding, as shown on the primary. In the ordinary design of electric filter circuits, the capacity of the end condensers, if we use the mid-shunt termination, should be one-half of that of the condensers in the centre. If there is only a single stage with the condenser at each end and one inductance in the middle, the two condensers should be equal. I should be very interested to know why he used the value of one-third, or whether it is merely empirical. With regard to Mr. Turner's point about the value of permeability and how to calculate it, I have done a certain amount of work on that subject, and particularly in regard to the change of inductance due to varying flux through the transformer. I attempted to calculate the inductance which would result by using a static iron characteristic, and I found that the a.c. inductance bore no relation to the position on the static iron characteristic due to d.c. flux. In

* Corrected for the *Journal* and the *Proceedings*.

general the d.c. flux tended to decrease the inductance rather than to increase it. The d.c. permeability of iron often varies from about 200 to 2 000. One would expect a.c. permeability to do the same, if one sets the d.c. flux to that point, but actually in practice it does not do so. I was particularly interested in the author's remarks about inter-electrode feed-back effects, as I have had considerable trouble in this direction in high-power modulation systems, where a number of valves are used in parallel and the inter-valve capacity is relatively large (of the order of 0·0001 to 0·0003 μF). In this case the feed-back effect is to produce a decrease in amplification at high frequencies; in some cases the amplification was reduced to 10 per cent of the maximum. With regard to the curves of transformers published in the paper, I should like to point out that, for good reproduction, frequencies as low as 50 cycles require amplification; and, no doubt, development in efficient transformer design has been considerably delayed by the fact that the average loud-speaker does not reproduce the low frequencies at all (i.e. the frequencies below 200 cycles). I think that future development should be on the lines of an essentially flat characteristic from 50 to 10 000 cycles, which, to my knowledge, cannot be obtained without considerable loss of efficiency.

Mr. C. G. Garton: I think that it is important to obtain as low values as possible in the leakage inductance and self-capacity, but it seems to be quite possible to get a very good design of transformer which has both large leakage inductance and large self-capacity. The curve can certainly be obtained flat to over 5 kilocycles. Can the author say whether he considers there is any great disadvantage in that design? I think that the second resonance point is probably about 6 to 7 kilocycles, rather than 9 to 10 kilocycles, which makes for a larger and more expensive design. But these figures are on the limits of the audio-frequency range, and I do not think the increased cost is justified by the better quality obtained by the author's design. With regard to the question of permeability of iron and the losses therein, so long as the a.c. amplitude is small compared with the d.c. polarizing current, there is no question that the differential permeability is the correct permeability to use. It is practically constant for the whole range of the curve until the saturation point is reached, and, for ordinary iron, ranges between 200 and 400. It is possible to obtain nickel-iron alloys in which the differential permeability reaches values as high as 1 200; but they are prohibitively expensive, unless it is required to make a special transformer for a particular purpose. There is very little difference between the permeability curves for 400 cycles and 50 cycles. I have not gone above 400, but I have found that the permeability is of very little importance above that frequency, because the reactance at 400 cycles is so large that one is not concerned with it but only with the question of iron permeability at lower frequencies. So far as the losses in iron are concerned, they seem to be negligible. As has been mentioned already, the inductance is the determining factor. I was interested also in the question of whether the bridge which is used for obtaining these frequency characteristics should or should not take account of the reaction due to the load in the second valve. My own bridge, an account of which was given in the *Journal*,* received precisely the same criticism. I wish to maintain strongly that these bridges ought to be worked with a second valve and no reaction. It is a sound principle in any investigation to reduce the number of variables as much as possible. If the load, i.e. the measuring valve of any bridge, is to be allowed to vary with the conditions, the characteristic curve obtained is not one for a particular transformer and its associated valve, but for a two-valve amplifier, in effect. Therefore, I think it is a sound principle to use a bridge which eliminates that effect and, if it does not give a practical curve, gives a curve from which something can be deduced, instead of one which is compounded of several different effects.

Mr. C. F. Phillips: Ingenious as the author's bridge arrangement undoubtedly is, from my own point of view it has certain disadvantages. Perhaps the most serious are the elaboration of the apparatus, the time taken in conducting a test, and the amount of arithmetic involved. I should like to indicate a method by which part of the information—the degree of effective amplification per stage—can be measured in a considerably simpler manner. This method has, however, one defect; it gives no information as to the comparative phase of the input and output voltages. It is in essence a variation of the old "slide-back" method, in which an a.c. voltage on the grid of a valve was measured by seeing how much bias was required to reduce anode current to zero. The serious disadvantage was the difficulty of determining just when the anode current became zero. Further, it cannot be used in this case, for the valves would be working under freak conditions. By watching, instead, the grid current, this method becomes quite practical. A galvanometer and potentiometer are inserted in the grid circuit of the valve before and the valve after the transformer, and each grid is biased until the grid current vanishes. Knowing the grid potential for zero grid current with *no* input E.M.F., one has only to observe the "drop" to give zero grid current in each valve under signals, and their ratio is the value of the stage amplification. It is obvious that the measurement is taken under precisely the right conditions for satisfactory amplification, and is therefore a true working test. In conclusion I should like to ask the author what, in his opinion, are the causes of transformer breakdowns.

Dr. R. L. Smith-Rose: It has been very noticeable during the last few years that new-comers to the intervalve transformer industry have, in nearly every case, repeated all the mistakes made by those who preceded them in the manufacture of such transformers. At the present time the available transformers can be divided into two classes, those which are moderately good and those which are bad. The author has gone about the matter scientifically, and has been studying the problem theoretically as well as from a practical point of view, and has thereby arrived at some quite successful results. One point which I should like to raise in connection with his method of testing, or in

* *Journal I.E.E.*, 1926, vol. 64, p. 270; and *Proceedings of the Wireless Section*, 1926, vol. 1, p. 29.

connection with the results obtained by testing, is the question of the lowest frequency at which he is able to carry out those tests. I understood previously that this frequency was in the neighbourhood of 250 cycles per sec., whereas in every case the curves in the author's diagrams continue down to 100 cycles per sec. Were those results obtained by extrapolation, or were they actual measurements with the bridge in question? Also, with reference to the question raised by Mr. Garton of studying the behaviour of an isolated transformer with no valve or its equivalent load attached, it should be remembered that at the present time the only practical application of intervalve transformers is as the coupling portion of a valve-amplifying stage. A perfect transformer may be produced, but in practice a valve must be connected to it with its associated load; and it is that combination which we desire to give uniform amplification at all frequencies. In the method of measurement described by Mr. Phillips, the only pitfall is the question of the limiting accuracy. The measurement depends upon the detection of the start of the grid current. Since the grid-current/grid-voltage curve meets the axis at a small angle, although not necessarily tangentially, it may be very difficult to determine the exact point at which the grid current commences, and that would place a limiting accuracy on the method. Considering that the transformer employs a closed iron circuit, I was surprised to find on pages 166 and 167 that the coefficient of coupling* is stated to be about 1 per cent. Is there not a slip in nomenclature here? The question of the future of the intervalve transformer to be used for reproduction of music appears to require concentrated attention in the direction of lower frequencies. If, instead of employing uniform frequency curves, the results are plotted on the pitch scale, one immediately realizes that the frequency of 250 cycles comes almost in the centre of the important part of the scale of audible frequencies. With frequencies above that limit we are able to obtain transformers with reasonably straight characteristics; but all those transformers show a tendency to fall off at a frequency below 250; and I think they must inevitably pass through zero at zero frequency. It is at the range of frequency between say 16 and 250 cycles that attention is now required, and I should like to obtain opinions as to whether we shall ever get a satisfactory coupling unit as a simple transformer or whether the final solution will not rather lie in the direction of a coupling component, using actual chokes and condensers the equivalent of which we are at present trying to concentrate into the windings of the transformer itself. I think it is possible that the development of such a coupling unit—it does not matter how complicated it is, provided it is put together in a box with just the input and output terminals accessible—will be necessary and more expedient in the very near future. In common with Mr. Phillips, I should like to hear the author's views on the subject of the breakdown of transformers. In this connection Fig. 19 shows one useful way out of the transformer breakdown difficulty by shifting the responsibility on to the choke, which is less expensive to replace; and in the case of a breakdown of the choke one can always use the transformer in its more usual circuit arrangement.

* Corrected for the *Journal* and the *Proceedings*.

Mr. P. W. Willans (*in reply*): I should like to thank the members of the Wireless Section for the kind reception which they have given to the paper and for the remarks of appreciation of the work carried out. The main criticism which has been directed against the work as a whole is that the bridge method does not give a true estimate of the performance of an intervalve transformer under the conditions of operation, and I propose therefore to deal with this point first of all, in reply particularly to the criticisms of Mr. Turner and Mr. Thomas. Here I am very considerably helped by the remarks of Mr. Garton who has been working on a similar method. First of all as far as the measurement of the performance of transformers is concerned we must necessarily employ some standardized conditions, and there is no doubt that if we allow the use of a subsequent valve with an arbitrary impedance in its anode circuit the conditions are the reverse of standardized. We are then concerned not only with the capacity of this valve, which may be regarded as of small account in determining the characteristic (apart from the Miller effect), but we must also consider the magnification factor and resistance of the valve and the nature of the impedance included in its anode circuit. I much regret that the measurements which have been given in illustration of the Miller effect were not available in the advance copies of the paper, but I hope that they give a sufficiently good indication that the bridge method of measurement is quite equal to taking this into account should it be desired to do so; and the fact that the main part of the paper has been written without reference to this effect, and only in connection with the performance of transformers under conditions where it is suppressed, is merely due to the very reasonable desire to make comparisons of transformer characteristics and constants under unvarying conditions. I cannot put this point better than it has been put by Mr. Garton in the latter part of his remarks.

With regard to Mr. Turner's further comments, some of the points he raises have been dealt with in the text of the paper, in particular the question of the permeability of the iron under the conditions of operation. This I estimated as having a value between 200 and 300 (cf. page 165, col. 2), and I note that Mr. Kirke and Mr. Garton both give values which substantially confirm my own. This permeability is of course the differential permeability at the points on the *B-H* characteristic corresponding to the value of the polarizing current under working conditions. For normal voltage-amplitudes this may be regarded as constant, though for high amplitudes at low frequencies there may be a deviation from this value, in general an increase. Mr. Garton has mentioned other types of iron, in particular nickel-iron alloys, and I can confirm the values of differential permeability which he gives, as also the question of expense.

With regard to the effective cross-section of iron, the work carried out does not give full information on this point, but it does give the practical information that the transformer can be regarded, between certain limits of frequency, as having a substantially constant

inductance at low flux densities. I would estimate these limits as between 250 and 1 000 cycles per second; at the higher frequencies the inductance may quite well fall off in value without this change being apparent from the transformer characteristics, since the inductive impedance is so very much higher than the capacitative impedance. For values below 250 cycles I have no measurements which are sufficiently reliable to give any indication. The leakage inductance of a transformer appears to rise slightly at the lower frequencies, but here its effect is not very great. From 1 000 cycles upwards it may be considered to have a constant value; but this is only natural, since one would expect the flux through the core to thread both windings and, in consequence, the leakage inductance to depend only upon the geometrical arrangement of the coils.

Mr. Turner's point regarding transformer losses has been dealt with on page 167 of the paper. These are negligible in any transformer of good construction unless there are short-circuited turns in the windings.

In reply to Mr. Turner's criticisms regarding the symbols used, I have only apologies to offer. I assure him that I will endeavour to "think internationally" in future. I have modified the text of the paper so as to meet his criticisms as regards the definition of Z and any lack of clarity there may have been as to what was meant by "anode voltage." The leakage of the transformer is of course 1 per cent and not the "coefficient of coupling," although I should like to mention, in passing, that if $1 - k^2 = 0 \cdot 01$ the value of k will be $0 \cdot 995$.

In reply to Mr. Thomas's criticisms, I have already dealt with the use of the bridge in relation to the Miller effect. With regard to his criticism of the method of measurement, I cannot agree that the "backing-off" valve, which I assume is the valve B′ in Fig. 3, acts virtually as a shunt to the normal output valve. It is quite clear from the manner in which the bridge is used that at the point of balance the plates of the valves B and B′ are at steady potential in relation to their filaments and also, by virtue of the Wagner balance, in relation to earth. It would seem that, provided the Wagner principle is properly applied, there is no possibility whatever of induction from one grid to the other through the plates of the valves. The effect of the large amount of apparatus, e.g. batteries and the like, is admittedly very serious unless a proper Wagner balance is employed; I have tried to use the bridge without the Wagner arrangement and obtained extremely inconsistent results, which resemble those described by Mr. Thomas. I admit that if an impedance be inserted in the anode circuit of the output valve and no trouble be taken to balance the bridge in the manner described, the load on the transformer may vary enormously and all sorts of discordant results be obtained. Without further knowledge of the exact conditions of Mr. Thomas's measurements I cannot suggest any explanation for his results, but I can assure him that the amplification of transformers is so closely in agreement with theory that there is not the slightest chance of an error as serious as 25 per cent. I have made no actual comparisons with other methods but have placed reliance on this agreement with theory, and I suggest that, if a comparison between any two methods is being made, the bridge be set up in exactly the manner described and an attempt be made to measure the input and output voltages of the stage of amplification at the same time. I regret that my resources have not been sufficient to make a really good comparative test on this point, and it would be of interest if Mr. Thomas or any other worker who has similar resources at his disposal could make a really satisfactory comparison. At the same time I feel that there must certainly be something wrong in the tests which he has so far carried out, and I venture to suggest that some consideration of the points above outlined may result in an explanation of the discrepancies experienced.

Mr. Kirke's remarks regarding the work which he has carried out on line transformers are of the greatest interest. These transformers are employed in circuits which are characterized by the presence of a load on each winding, and under practical conditions their self-capacity is so low as to be negligible. The result is a great predominance of the virtual capacity term due to losses and leakage and, in consequence, a state of affairs where it is difficult to get adequate reproduction of the lower frequencies without a cut-off at the higher frequencies. Mr. Kirke's success in this respect is of course well known. There seems to be some slight confusion regarding the question of amplitude distortion in the series condenser circuit, as Mr. Kirke alludes to the variation of output impedance due to the condenser and transformer primary forming a series resonant circuit, which of course is the principle underlying the method whereby an accentuation of the lower frequencies is obtained. My contention was that if the input to the valve varied, the effective inductance also varied, and the anode circuit impedance in consequence became a function of the amplitude. Mr. Kirke appears to put this in other words by saying that the dynamic characteristic will vary considerably and so cause distortion. Such distortion would appear to me to be inevitable at some low frequency and, in view of the amplification rising to a high peak value in the series condenser circuit, I consider the latter is more prone to give variations in the dynamic characteristic than a simple transformer. This defect is not found in the reaction circuit described in the paper. Mr. Kirke's notes on the subject of rectifiers are of great interest; I have noticed that a condenser and leak in series with the grid of an amplifying valve produce quite a definite change in the amplification of the transformer following it. With regard to the filter-circuit question, I am afraid that this was dealt with in a very inadequate manner when the paper was read and I have therefore elaborated it somewhat in the Appendix. The point really is that the arrangement is strictly not a filter at all but only analogous to one, in that it is a device for producing a constant voltage across a given terminal impedance when the input impedance is a resistance of given value. The value of one-third is based entirely on theoretical considerations, as appears from the analysis given in the Appendix. Mr. Kirke notes the necessity for the adequate reproduction of really low frequency, and this point is also mentioned in Dr. Smith-Rose's criticisms. I have dealt with this question later in my reply.

In reply to Mr. Garton I am bound to say that I consider it advisable to have the high value of second resonance which was specified, as I believe that under these circumstances the transformers work better in combination with each other whether the neutrodyne method be used or not. If a neutrodyne connection is employed, low leakage inductance is essential in order to get adequate neutralization with valves of high magnification factor, and it is definitely possible to employ such valves without loss of quality. If no neutralization be used it is necessary to reduce the step-up ratio of the transformer, and in this case the effect of the reaction from a subsequent stage of amplification will be to reduce the extent of possible straight characteristic by steepening the drop of P. I feel very strongly that there is a necessity for a margin of straight characteristic over and above what might be regarded as the upper limit of practical audio frequency in order that there may be something in hand when the transformers are worked in association with each other. With the remainder of Mr. Garton's remarks I am in entire agreement, and the points he raises have already been dealt with.

Mr. Phillips's criticisms of the bridge method on the grounds of the difficulty of working it are, I think, unjustified. I admit that the arrangement looks somewhat terrifying in the form of a circuit diagram, but in practice it is readily possible to take, say, six observations of a transformer amplification curve in the space of 20 minutes or so, and the reduction of these results when reduced by a rule-of-thumb process is very speedy indeed. Furthermore, the possibility of extrapolating the results and of deducing what is going on in the circuits under consideration, to my mind far outweighs any preliminary difficulties in getting acquainted with the routine of working the apparatus. With regard to Mr. Phillips's suggested method, I share Dr. Smith-Rose's mistrust of the slide-back principle, particularly in view of the possible damping effect of grid current. I have used slide-back methods in the past and have found the observation of the " end point " an extremely difficult and nerve-racking matter, and it would seem barely worth while to place reliance on such a method now that the thermionic voltmeter has reached such a satisfactory state of development. A possible source of error in all methods of this kind may be worth mentioning, namely, that due either to bad wave-form of the input frequency or to the generation of harmonics in the circuit under test. The bridge method has the advantage of isolating one frequency in what is possibly the simplest of all manners. With regard to Mr. Phillips's question on the subject of transformer breakdowns, which are also alluded to by Dr. Smith-Rose, I fear that I have no useful information to give, though I believe that with the transformer under discussion a satisfactory method of impregnating the windings has been arrived at and breakdowns now no longer occur. Unfortunately, as I was no longer connected with this work when the trouble of breakdowns arose, I have not had any experience in this respect and can therefore make no useful contribution to the question.

In reply to Dr. Smith-Rose, the measurements down to 100 cycles per second are extrapolated, though I see no reason why the bridge method should not be applied to such frequencies if a vibration galvanometer is substituted for the telephones. I appreciate that we have to consider the effect of a loaded transformer, but I submit that the use of the neutrodyne renders the associated load practically negligible at the important audio frequencies, and that we may consider this combination just as well as any other in dealing with the transformer problem. Apart from the question of neutralizing, there is no question to my mind that a transformer which is good when unloaded is also good when loaded. The nomenclature as regards the coefficient of coupling was of course a slip and has been rectified for the *Journal*. Dr. Smith-Rose's remarks on the importance of low frequencies are of great interest, though as a result of comparisons between resistance and transformer amplifiers I would not agree that the frequencies below 250 cycles are of as great importance as the frequencies above this limit. I am considering now purely the æsthetic aspect of broadcast reproduction, and I can only explain this by the fact (for so it appears to me) that if we are enabled to reproduce all the harmonics of a given tone in an adequate manner we get psychologically a large measure of the effect of the fundamental note. I believe that experiments have established this to a remarkable degree. However, there is no doubt that the lower frequencies have great importance as far as naturalness of certain tones is concerned, and I quite admit that the transformer in question, in common with nearly all others, leaves a considerable amount to be desired in this respect. I believe, all the same, that there is great hope in the correct application of the low-frequency reaction principle to supplement the lower frequencies, on the lines of the circuit which has been described in the paper.